

Overview

We are extremely grateful to the reviewers for their careful and thoughtful reviews of our paper, and for their support. They raise some excellent points, which we respond to individually below. The reviewer comments are shown in black text, with our responses in blue.

Franka Jesse

General comments

This manuscript presents a set of high-resolution (400 m horizontal, 10 m vertical) three-dimensional ocean model simulations of the Amundsen Sea region, with a particular focus on the Pine Island, Thwaites, and Dotson–Crosson ice shelves. Simulations representing the present-day ice-shelf geometry reveal melting hotspots beneath the deepest ice drafts associated with the main ice streams, as well as in regions characterised by buoyant meltwater outflow. The authors then compare the modelled melt patterns with those produced by nine simple "local" ice-shelf melt parameterisations. Here, "local" refers to parameterisations that combine far-field ocean temperature with only local geometric properties of the ice shelf, namely ice draft and basal slope, to estimate melt rates. They find that a parameterisation scaling with ocean temperature to the power of 3/2, ice-shelf slope to the power of 1/2, and incorporating tapered melt rates near the grounding line provides the highest spatial pattern correlation with the ocean model results. This parameterisation successfully reproduces the major melting hotspots and broadly captures the enhanced melting associated with evolving ice-shelf geometry, although it does not reproduce the enhanced melting driven by buoyant meltwater outflow. By systematically evaluating the tested parameterisations against high-resolution ocean model simulations, the study provides a valuable benchmark for the ice-sheet modelling community, particularly for researchers working on the Amundsen Sea sector.

I found the manuscript to be well written and easy to follow. The experimental design is clearly described, and the development of the parameterisations is presented in a logical and well-structured manner. The figures are of high quality and effectively support the discussion. I also greatly appreciate the authors' effort to make the ocean model output publicly available and to explicitly encourage their use by the broader research community.

We are extremely grateful to the reviewer for their careful reading and support of our study. The comments have substantially improved the paper.

Below, I first outline two more substantial points that I believe should be addressed to further strengthen the manuscript. I then provide specific line-by-line comments, followed by some minor technical corrections. Overall, I recommend minor revisions.

1) The authors conclude that Eq. 18 (first row in Table 2) provides the best performance. However, based on Table 2, the quadratic parameterisation without slope dependence but including grounding-line tapering ($m = C(T^r - T_f^r)^2 \Psi(3km)$) appears to perform nearly as well. It has only a slightly lower r^2 -value while achieving a lower Mean Absolute Error. This parameterisation is not discussed in Section 3.4 and is also absent from Figure 7 (which only shows the quadratic parameterisation without grounding-line tapering). Since this parameterisation is very similar to the quadratic parameterisation currently used in many ice-sheet models, I think the improvement obtained by introducing grounding-line tapering is of particular interest to the ice-sheet modelling community.

The authors should therefore include this parameterisation in both the discussion and Figure 7, as an explicit comparison with Eq. 18 would help readers assess the trade-offs between the two approaches.

This is an important observation and the presentation of this tapered quadratic formulation will be improved in the revised manuscript.

In the submitted manuscript, this formulation was less emphasised for three reasons. Firstly, we argue that any formulation that is quadratic in temperature has no clear theoretical basis unless it is also linearly dependent on the ice base slope, and for that reason we prefer the expression (18) – see sections 3.4 and 4.2. Secondly, we have some reservations about the tapering, which may improve the fit to modelled melt rates simply because the model is unable to represent melt rates in the narrow cavities near grounding lines that would occur in reality – see section 4.2. Finally, the benefit of tapering is quite uniform across all parameterisations, and so we present only the untapered results in Figure 7 for most parameterisations in order to simplify that figure.

We agree with the reviewer that this formulation should receive more attention in the revised paper, and so we will expand sections 3.4 and 4.2 to include these arguments. However, we prefer not to add this formulation to Figure 7. Figure R1 shows what that figure would look like. From this figure it is clear that the tapering has little effect on the overall melt volume flux, and

offers a similar improvement in correlation for both the quadratic law and the favoured expression (18). We feel that adding this additional line does increase the complexity in the figure, and so we will instead note these points in the text in the revised sections 3.4 and 4.2.

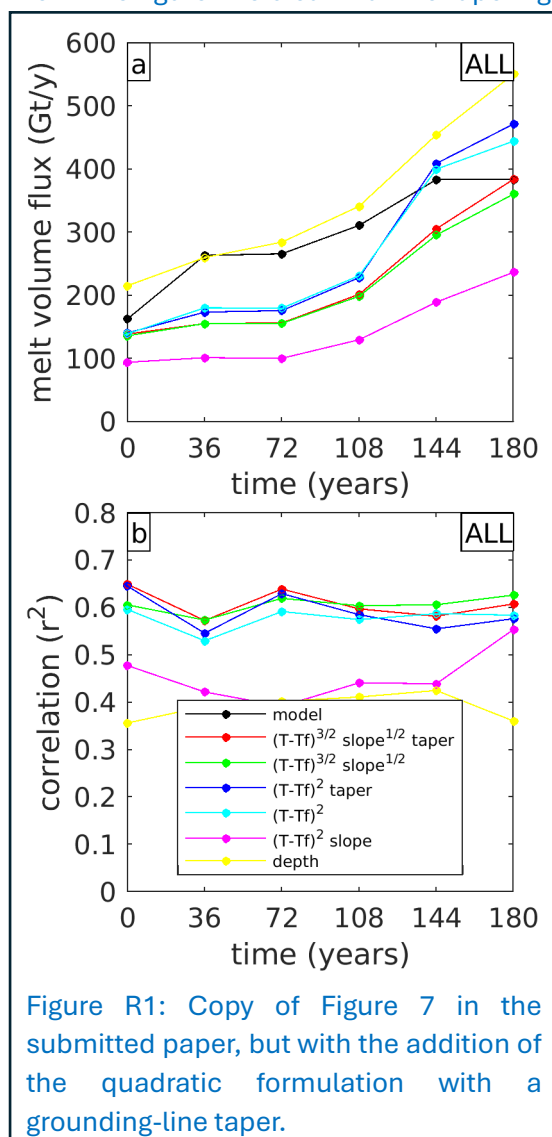


Figure R1: Copy of Figure 7 in the submitted paper, but with the addition of the quadratic formulation with a grounding-line taper.

2) I appreciate that the authors evaluate the parameterisations across a range of ice-shelf geometries, which increases confidence in their robustness to geometric variations. However, I think the manuscript would be further strengthened by also assessing their sensitivity to variations in the far-field ocean forcing, particularly given the stated goal that these parameterisations could replace the need for a coupled ocean model. This aspect is especially important because the parameterisations exhibit different dependencies on thermal forcing (e.g. powers of $3/2$ versus 2), suggesting that they respond differently to changes in ocean temperature. As currently presented, it is difficult to assess whether the conclusions are robust beyond the chosen forcing scenario.

Therefore, to address this, the authors should:

- Preferably, run one or two additional MITGcm simulations using the present-day geometry with slightly warmer and/or colder ocean temperature forcing. This would allow for a direct assessment of the robustness of the parameterisations under different ocean conditions and provide insight into whether the calibrated coefficient (C) remains transferable across different forcing scenarios.
- Alternatively, if such additional simulations are not feasible, expand the Discussion section with a more comprehensive discussion of this limitation (at least a full paragraph), explicitly addressing how differences in thermal forcing dependence may affect the applicability of the parameterisations under changing ocean conditions.

This is an excellent suggestion and something we had considered before submitting the paper, but decided not to pursue through lack of time. The reviewer inspired us to revisit this decision and we ran a set of additional simulations, using the present-day ice geometry and with the thermocline height varied in the boundary conditions, to provide a thicker or thinner layer of warm Circumpolar Deep Water. This modulation in the deep warm layer is the main mode of ocean variability in the Amundsen Sea, and varying the thermocline depth in this way is a standard approach in modelling studies (e.g. De Rydt et al., 2014; Holland et al., 2023). The reference present-day thermocline depth is 700 m, as used throughout the submitted paper, and so we varied this between 900 m and 400 m in 100 m increments, to represent both cooler and warmer conditions.

Figures R2 and R3 show the result of these simulations. Figure R2a shows the modelled melting (black) and the result of the different parameterisations (colour), where each parameterisation was calibrated to fit the

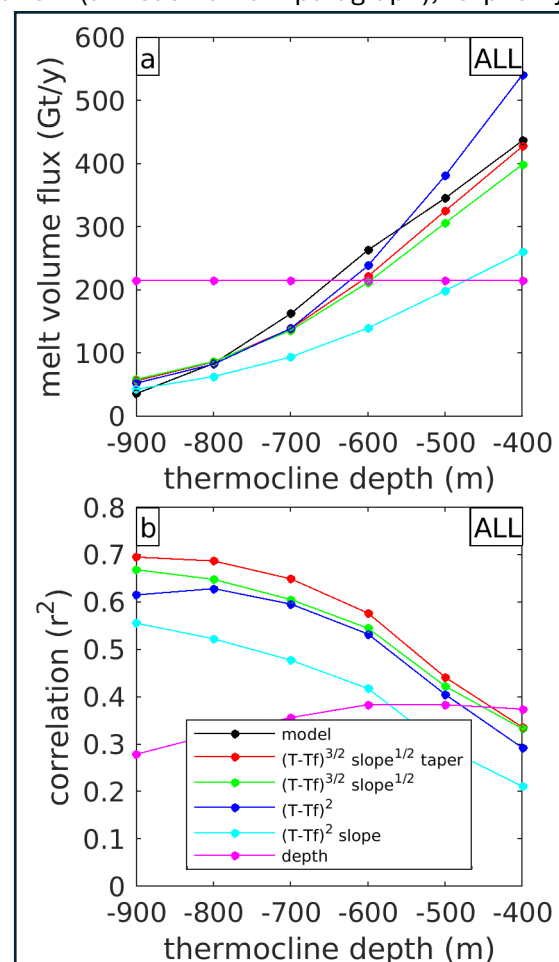


Figure R2: a) Response of ice shelf melting to variation in thermocline depth, integrated across all ice shelves; b) performance of parameterisations as

reference present-day 700 m thermocline case, as described in the submitted paper. It is again visually apparent that the favoured expression (18) produces the best match to the overall melt volume flux sensitivity to thermocline depth variations. Figure R2b shows the pattern correlation for the different parameterisations, again confirming that expression (18) maintains the best correlation for all thermocline depths. Importantly for the paper, the quadratic expression performs less well.

Figure R2b also shows an important result that the skill of most of the parameterisations declines as the thermocline is raised. Figure R3 shows why this is. In the model results, the deep melting hotspots get more intense as the thermocline rises. However, this effect is muted in the

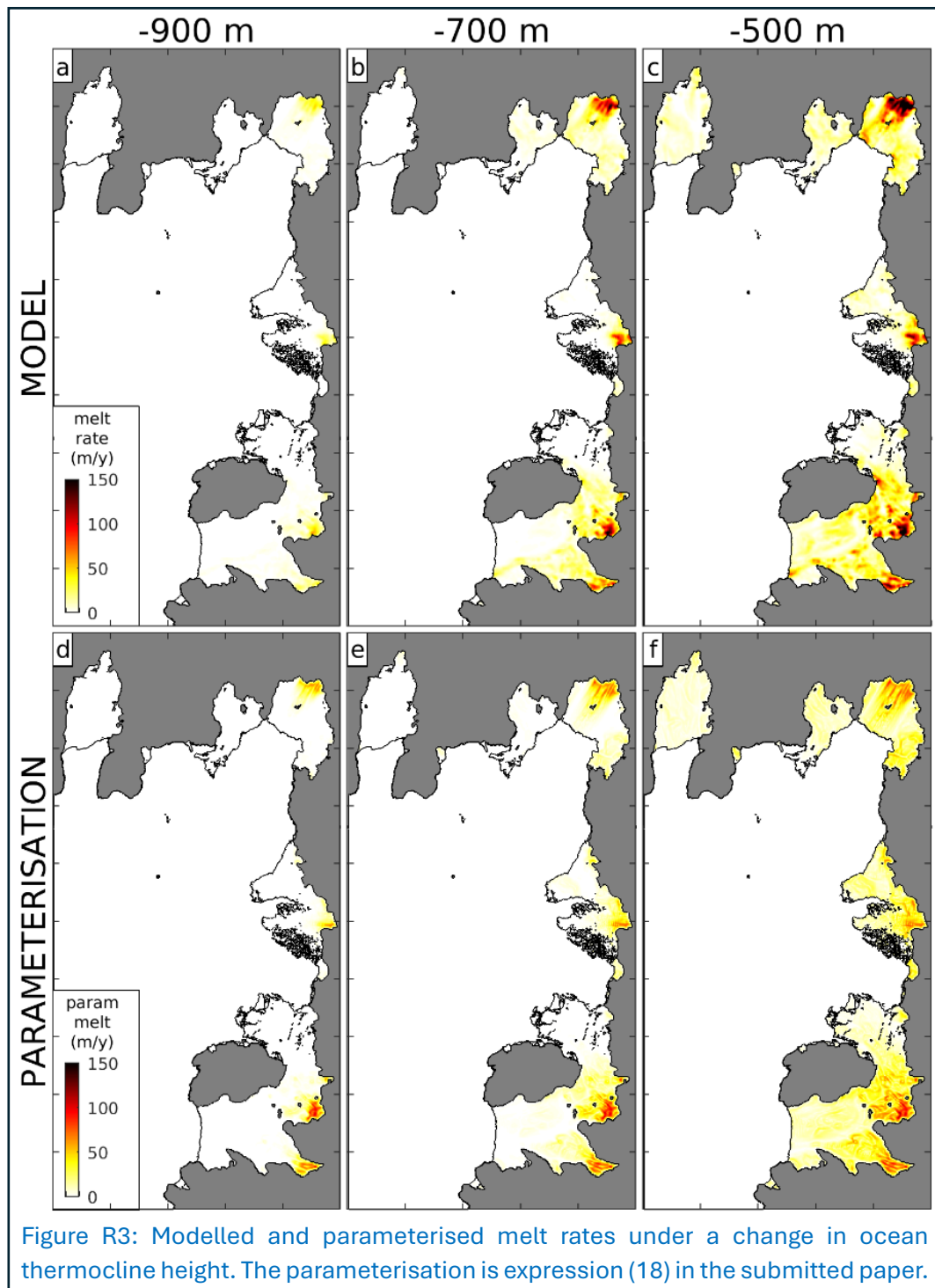


Figure R3: Modelled and parameterised melt rates under a change in ocean thermocline height. The parameterisation is expression (18) in the submitted paper.

parameterisations. This is because the deep ocean temperature below the thermocline is held fixed, and therefore the parameterised melting in this deep layer does not change at all, unlike in the simulations. Improving the parameterisations in this regard would require some representation of the deep water flow over seabed sills in the parameterisation, which is an important discussion point.

We think these are really valuable results to add to the paper and we thank the reviewer for encouraging us to run these simulations. We will describe these new forcings in section 2.1.3. We will to add Figures R2 and R3 to the paper, as part of a new results section 3.4. A description of the parameterisation ability to capture variations in thermocline height will be added to the Discussion, Conclusions, and Abstract.

Specific comments:

l. 10/24/35/51: I suggest replacing "ocean melting" with "ice-shelf melting" (like in the title) or "ocean-induced melting", as these terms more accurately describe the underlying physical process.

Changed variously to 'ocean-driven ice-shelf melting' etc.

l. 14: From the abstract, it is not directly clear what is meant by "local". I suggest clarifying this (e.g., simple parameterisations of melt that depend only on local ice-shelf geometry variables).

Changed to 'The simulations are then used to test simple 'local' parameterisations of melting that depend only on local ice geometry and far-field ocean properties.'

l. 14-16: Could you specify what is meant by "the best parameterisation"? As written, it is not immediately clear that this refers to reproducing the spatial melt pattern. While the following sentence hints at this, I suggest making it explicit from the start.

This sentence is already long and complex, so instead the following sentence is made more explicit that the melting pattern is the target.

l. 17-18: I suggest referring to "the ocean model" rather than "the model" for clarity.

Changed

l. 48-49: This phrase is slightly repetitive and difficult to read. I suggest revising to "[...] implication: constraining projections of [...]".

Changed

l. 124: Could you add a reference that supports "within 2.5 ice thicknesses (a typical ice bending length scale)"?

This is an empirical result based on the observed deflection of ice shelves, across many studies, though it is very approximate. Sayag and Worster (2011) suggest that the ice bending—buoyancy length scale varies like the ice thickness to the power $\frac{3}{4}$, with the precise value depending upon the elastic modulus and Poisson ratio of the ice. Conley et al. (in preparation) test this concept in the Amundsen Sea. We added 'In this study we make a rough approximation that the bending length scale is 2.5 times the ice thickness (Sayag and Worster, 2011; Conley et al., in preparation).'

l. 140: Could you specify the shape of the "thermocline", e.g. "linear thermocline" ?

Changed

l. 180: Please add a reference at “[...] is the following set of equations”

Added a citation to Jenkins et al. (2010)

l. 180-213: The terminology in Section 2.2.2 is somewhat unclear regarding what is meant by “the model”, and I find that “melt formulation” and “melt parameterisation” are not used consistently. In particular, “parameterisation” does not always appear to refer to the newly developed parameterisations, but is sometimes used for the melt formulation used in MITgcm. It would be helpful to explicitly state that the local melt parameterisations (all of which take the form of Eq. 20) are developed starting from the melt formulation implemented in MITgcm. To improve clarity, I suggest the following specific changes:

- In l. 180, replace “the model” by “MITgcm” or “the ocean model”.

Changed to ‘the ocean model’

- In l. 191, replace “the model” by “MITgcm”

Changed to ‘the ocean model’

- In l. 195, replace “this parameterisation” by “this formulation”

Changed

- In l. 199, replace “in the melting parameterisation” by “in the melting formulation”

Changed

- In l. 208, consider removing “which underpins all parameterisations that follow”, as this is potentially confusing given that the equation represents the underlying formulation rather than the later parameterisations introduced in the manuscript; the subsequent text l. 210-213 already explains its role.

Removed

- Table 1, for φ , change description from “melting multiplier used to tune melting parameterisation” to “melting multiplier used to tune melting formulation”

Changed

l. 342: The text states that δx is varied between 0 and 5 km, whereas Table 2 only shows experiments with either $\delta x=0$ or $\delta x=3$ km. I suggest revising this statement to reflect the presented setup, e.g. “ δx is either 0 or 3 km.”

Changed

l. 343-344: Could you clarify the phrase “[...] basis in physical theory” by explicitly referring back to Eqs. 18 and 19, which appear to underpin this statement?

Changed

l. 348: For clarity, I suggest replacing “all ice shelves Dotson–PIG” with “the Dotson–Crosson, Thwaites, and PIG ice shelves”. The shorthand “Dotson–PIG” is not introduced elsewhere in the manuscript so spelling out the individual ice shelves would make the sentence easier to follow and immediately clarify that Cosgrove Ice Shelf is not included in the tuning.

Changed

l. 365-368: The sentence “We only model [...] ocean-induced glacier change (references)” appears to describe a methodological choice rather than a result or interpretation. I suggest moving this sentence to the Methods section, perhaps to Section 2.1.1, where it would fit more naturally.

The opening sentences of section 2.1.1 and section 3.1 were rewritten accordingly

l. 380-382: The sentence “The extremely high melt rates in these hot spots arise because the deep ice base is sitting in the warm Circumpolar Deep Water layer, and hence has strong thermal forcing, and is also steeply sloping, and hence has strong buoyancy-driven meltwater currents” is somewhat difficult to read in its current form. I suggest rephrasing or splitting for clarity, e.g. by separating the two mechanisms (thermal forcing and slope-driven circulation).

This is not straightforward because this sentence already sits within a numbered list within the paragraph. We decided to add parentheses: ‘The extremely high melt rates in these hot spots arise because the deep ice base sits in the warm Circumpolar Deep Water layer (hence has strong thermal forcing) and the ice base is steeply sloping (hence has strong buoyancy-driven meltwater currents).’

l. 410: Please add a reference supporting the claim that portions of the ice may be held out of flotation due to ice-bridging stresses.

Added citations to Sayag and Worster (2011) and Conley et al. (in preparation).

l. 444–451: From “Before proceeding, [...]” onwards, this paragraph reads more like a description of the methodology than a presentation of the results. I suggest moving this part to Section 2.2.8, where it would fit more naturally. A brief reference to this aspect could then be retained here.

Moved to last paragraph of section 2.2.8, and merged into first paragraph of section 3.2.

l. 458: Regarding the sentence “The best-performing parameterisation is found to be expression (18) [...]”, I do not think it is strictly correct to refer simply to expression (18), as the evaluated parameterisation also includes the tuned value for C and the specific tapering scale = 3 km. It may therefore be clearer to refer to “the parameterisation using the same ocean temperature and slope scaling as in expression (18) (i.e. dominated by buoyancy-drag balance)” rather than simply “expression (18).” This happens at several occasions (l. 464, 529, 545, 554, 610, 630). You could consider numbering the parameterisations in Table 2 such that you can simply refer to the number.

We now state this equation in the text as the new equation (21), and refer to that thereafter.

l. 483-484: I am not sure I understand the sentence starting with “The field plotted [...]”. As written, it suggests that $(T^r - T_f^r)$ is optimally scaled to match the modelled thermal driving. However, my understanding is that the scaling (which is absorbed into the tuning coefficient C) is instead optimised to match the modelled melt rates, rather than the thermal driving itself. In other words, C combines the scaling of thermal forcing and friction velocity into a single tuned coefficient, which is calibrated against melt rates. Could you clarify this point and, if necessary, revise the sentence for clarity? If I have interpreted the methodology correctly, it seems that “melt rates” would be more appropriate than “thermal driving” in this sentence.

The fields shown in Figures 4e and 4f are actually the result of separate parameterisation fitting and evaluation procedures. They do not result from the main parameter tuning of the whole melt rate parameterisation. So the reviewer's initial impression ('As written...') is correct. We apologise for our lack of clarity here and have now tried to make this clearer by rewriting the paragraph:

'The bottom row of Figure 4 illustrates the parameterised analogue of each of these three quantities, as implied by the structure of (18). Each of the three panels Fig. 4d-f is created by a separate parameter-fitting exercise. The parameterised melting (Fig. 4d) is created according to the fitting procedure described previously and is exactly as shown in Fig. 3. The parameterised thermal driving plotted in Fig. 4e is the result of optimally scaling $(T^r - T_f^r)$ to match the modelled thermal driving $(T - T_b)$ over all ice shelves ...'

l. 525: From Figure 5, the peak melt rates near the grounding line appear to differ by approximately 50–100 m/yr. I therefore suggest replacing "[...] albeit with slightly less intense peak melt rates" with "[...] albeit with less intense peak melt rates", as "slightly" seems to understate the difference.

Changed

l. 565-582: There is considerable overlap between these two paragraphs and the text in Section 3.1 (l. 375-431), and Figure 8 covers almost the same information as Figure 2. I suggest merging this text into Section 3.1 and incorporating the contours shown in Figure 8 into Figure 2. In my view, the additional contours would not distract from the message of Section 3.1; rather, they would complement and strengthen it.

The remainder of Section 4.1 focuses on the implications for ice loss, which I think would work well as a discussion section on its own.

We considered this before submission, and revisited this in light of the reviewer's comment. There is a strong argument for combining the figures, but we prefer to leave the structure as it is, for the following reasons:

- ➔ This text serves as the Discussion section of the paper. This is where we start to synthesise the results and discuss their implications for the grounded ice sheet, and we feel that the top-level conclusions of 'hot spots' and 'meltwater outflows' are worth the repetition.
- ➔ In section 4.1 we seek to explain that the influence of ocean-driven melting on ice sheets occurs through both the mass balance (Fig. 8) and the momentum balance (Fig. 9). The mass balance arguments require the new contours of total mass flux.
- ➔ In section 3.1 we are at the stage of basically describing the results of the model, and in that section we investigate each ice shelf cavity in turn, examining the details of the circulation and melting, rather than seeking to synthesise the results across the whole sector.
- ➔ For display purposes, we prefer to keep Fig. 8 separate from Fig. 2 because we want to present this result at this later stage of the paper, not earlier, and keeping it separate means that the panels can be larger, so that all of the features are more easily visible.

Fig. 9: I suggest adding to the caption that the mask and ice-shelf front shown are derived from this study, whereas the datasets from other studies may use slightly different geometries, including calving front and grounding-line positions. This would help explain some of the white areas near deep grounding-line regions.

Changed

l. 621-622: Replace “this high-resolution model” with “the high-resolution model employed in this study” for clarity.

Changed

l. 634: Should “overall form of (19)” not read “overall form of (18) and (19)”? Or does it exclusively apply to (19) and why so?

It applies to both and so (18) has been added

l. 663: Could you clarify what is meant by “undesirable consequences”?

Clarified to ‘that the parameterisation structures recommended here may lead to undesirable results when used to drive an ice sheet model, and this remains to be tested.’

l. 676: It would be helpful to clarify what is meant by “local” (and, in my view, this could also be clarified in the abstract; see comment about l. 14). Defining this term more explicitly would immediately indicate the scope of the comparison, for example that parameterisations such as PICO or plume-based approaches are not included.

Changed to match abstract

Technical corrections

l. 94: Please define the acronym PIG at its first occurrence.

Changed

l. 601: I believe Fig. 8 should be Fig. 9.

Changed

l. 627: I believe Table 1 should be Table 2.

Changed

Figures: The same colormap is used for different variables. In Fig. 4 this is understandable, as the variables are correlated. However, in Figs. 1 and 5 it may improve readability to use different colormaps to avoid potential confusion. For example, a separate colormap for temperature maxima and bed elevation in Fig. 1, and distinct colormaps for draft and melt rates in Fig. 5.

We experimented with a wide variety of colormaps before submission and on further consideration we still feel that the chosen colormaps best illustrate our conclusions.

Equations: I am not sure about the equation style in TC, but several equations appear to be missing commas and punctuation. It may be worth checking this throughout. For example, at l. 209–210, a comma could be added after the equation and a lowercase “where” used to introduce the definitions (i.e., “[equation], where [...]”).

Changed

Anonymous Referee #2

The authors of this manuscript first review the importance and conceptual background for simple ice-shelf melting parameterisations. Then, they propose new expressions for simple parameterisations and evaluate them in comparison to high-resolution (400m) ocean simulations of the Eastern Amundsen Sea. They show best performance for the parameterisation that expresses melt rate as a function of ocean temperature to the power $3/2$ and ice slope to the power $1/2$.

This manuscript is very well written, and the theoretical considerations on melt parameterisations (section 2.2) are very clear and interesting in and of themselves. The comparison with the high-resolution model is convincing and the caveats are clearly stated. Improving ice-shelf melting parameterisations has a large potential impact on the quality of ice-sheet and sea-level projections.

[We are very grateful to the reviewer for their careful reading and support of our study](#)

I recommend the acceptance of this manuscript for publication in The Cryosphere, and I just have a few minor comments that the authors may or may not address.

Minor comments.

L. 94: expand “PIG” at first use.

[Changed](#)

Section 2.1.1: please indicate a model version or a repository of the used code to ensure future interpretability if important parts of the code happen to change substantially.

[This information has been added to the code and data availability section](#)

Figs. 2, 3, 4: the captions should indicate that the figures are obtained from a single simulation and specify from which T,S restoring profile.

[Changed to highlight that the plots are of the reference present-day simulation.](#)

L. 344-351: Is the model-parameterisation misfit calculated for all grid points and all sets of T,S restoring profiles? This could be more explicit.

[Changed to say that all grid cells are used and it is the reference present-day simulation](#)

L. 380: at the first use of “m/y” as melt rate units, it would be good to link to standard units, with something like “100 m/y (meters of ice per year: 1 m/y = 917 kg/m²/y)”.

[This is an excellent point. In the submitted paper, all melt rates were expressed as freshwater equivalent \(using a density of 1000 kg / m³\). We will change this to ice density and note this choice in the text.](#)

L. 436: “We only consider the time-mean melt rates, neglecting the influence of turbulent eddies”. I don’t think this is completely true. If you average melt rates, then you average the influence of eddies rather than neglecting their influence (e.g. a linear melting law is proportional to $\langle uT \rangle = \langle u'T' \rangle + \langle u \rangle \langle T \rangle$ with $\langle u'T' \rangle$ including the effect of eddies). A more important caveat is probably the absence of tides, both for their direct contribution to ocean currents and for the intrusion of seawater into the grounding zone as the ice shelf moves up and down (Mamer et al. 2025).

[We have removed the comment about eddies and replaced it with a mention of tides and a citation to Jourdain et al. \(2019\).](#)

“correlation”, “coefficient of best fit” and “coefficient of determination” are all used to refer to r^2 . It would be easier to read with single naming.

The ‘coefficient of best fit’ is the derived slope of the linear relationship between two quantities. This phrase is only used once on line 487 so is expanded there. The distinction between the ‘correlation’ and the ‘coefficient of determination’ is potentially confusing. They are not the same thing because there is no constant term in the parameterisations and hence no intercept allowed in the linear fitting, as described in section 2.2.8. We now describe this issue more fully in that section, and use the term ‘pattern correlation’ throughout the paper.

Section 3.4: A comment on the poor result of the $m = C z^b$ parameterisation would be welcome.

Added “This is because the hot spots lead to melting that increases non-linearly with depth, while the complex meltwater outflows cause a large spread in melt rates at each given depth.”

The discussion could include a small paragraph on the simplification obtained from using constant and uniform T,S restoring profiles in the simulations. In a more realistic application, spatial and temporal variations make it less obvious to choose where to sample the far-field ocean properties. This is nonetheless unlikely to affect the comparisons between the different parameterisations.

This important point has been added to the end of section 4.2

Reference:

Mamer, M. S., Robel, A. A., Lai, C. C., Wilson, E. and Washam, P. (2025). Modeling mixing and melting in laminar seawater intrusions under grounded ice. *The Cryosphere*, 9(8), 3227-3251.

References

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